



COMMENTARY

Not So Fast: A Response to Augustine's Critique of the BICS Contest

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HIGHLIGHTS

A prior cynical review of the outcomes of a recent contest on the best evidence for “life after death” arguably rehashes many familiar and trivial criticisms of paranormal research. Problems with survival-type studies exist, but some evidence seems much stronger than what skeptics assume or conclude.

ABSTRACT

Keith Augustine’s critical evaluation of the essay contest sponsored by the Bigelow Institute of Consciousness Studies (BICS) is an interesting but problematic review. It mixes reasonable and detailed criticisms of the contest and many of the winning essays with a disappointing reliance on some of the most trite and superficial criticisms of parapsychological research. Ironically, Augustine criticizes the winning essays for using straw-man arguments and cherry-picked evidence even though many of his own arguments commit these same errors.

Preliminaries

Augustine’s detailed essay is an interesting—and at times, frustrating—stew. It mixes reasonable criticisms of the BICS contest and many of the winning essays with lazy reliance on some of the most tired and shallow critiques of parapsychological research.

We agree that Augustine has identified some areas of concern about the BICS contests’ design and execution. One of those is BICS’s controversial reliance on the legal standard of proof beyond reasonable doubt. Moreover, Augustine provides reasons for questioning both whether the judging was sufficiently objective, and also whether judges (even competent and objective judges) could have properly evaluated the enormous body of submissions in the time allotted for that task. Granted, the BICS contest may have successfully and commendably brought widespread attention to the fact that serious survival research

and scholarship exist. But it did not discover or create an authoritative consensus about what the “best” evidence is, much less clarify the principles by which ostensible survival evidence should be evaluated. Augustine argues that it would have been better to “commission an evidence review (not an essay contest) by *independent* judges, such as those in the biomedical field who have *not* published in the survival literature, to avoid potential conflicts of interest.”

Our response to Augustine’s critique has some unavoidable limitations. Augustine offers many criticisms of the winning BICS entries he selected for discussion, and we cannot assess them all. In fact, we prefer to shelve discussion of the messy particulars in Augustine’s selection of essays, thereby sparing the reader from being drenched in minutiae. Besides, there are bigger concerns that take priority. We need to examine major and pervasive deficiencies in Augustine’s discussion—for example, his reliance on straw-man or other notoriously unacceptable tactics, his



refusal even to *mention* positive evidence, and his failure to realize that there is nothing privileged about the many assumptions he brings to the table.

The Big Picture

As we will see, convenient lacunae in Augustine's critique allow him to make his skeptical position seem more substantive than it really is. For example, Augustine carefully avoids discussing two matters of great importance: (1) not simply the strongest reasons, but any reasons for challenging his negative appraisal of particular cases, and (2) arguments exposing how unverified assumptions and hasty inferences pollute the received view of the relevant physiological data. In fact, Augustine entirely ignores McTaggart's subtle re-framing of the issues (discussed below and in Braude, 2003), showing how we can modify our language to purge it of some conventional metaphysical presuppositions, and thereby allow us to see the physiological evidence in a more survival-friendly light, as if through a different metaphysical lens.

Turning now to the details of what Augustine says, consider the following illustration of point (1) above. Augustine writes,

. . . the fact that historical trance mediums' accurate statements must be fished out of reams of twaddle (James, 1909, p. 115) is surely relevant to any plausibility assessments here, as is the agreed-upon fact that a significant proportion of the entities that they claimed to contact were undeniably fictitious constructions of the mediums' own minds. Certainly the latter more than offsets any gain provided by appealing to the "never caught cheating" card, which is hardly conclusive in any case since Mrs. Piper had access to gossip within a large web of her community connections.

This passage commits several sins. First, as far as clearly fictitious mediumistic control personalities are concerned, even if one grants the reality of survival, the existence of these controls would not be surprising. They might even be exactly what many survivalists expect. An extensive body of research, primarily studies of hypnosis and personality disorders (especially MPD/DID), reveals how dissociative and other altered states can unleash remarkable displays of creativity or previously latent abilities, including precisely the kind of creative achievements seen in mediumistic control personalities (as in the case of Patience Worth—see Braude, 2003). Augustine ignores those bodies of work, apparently unaware of the complexities of dealing with what Braude (2003) has called the *Un-*

usual Suspects—namely, rare or abnormal processes, such as a combination of dissociation and latent creative capacities, or exceptional (e.g., "photographic") memory, or something analogous to extreme or rare forms of savantism, where we find remarkable skills existing alongside cognitive and physical deficits that ordinarily prevent the manifestation of those skills.

Moreover, in the passage above, Augustine tries to dismiss Mrs. Piper's case with an undefended appeal to the medium's presumed access to "gossip within a large web of . . . community connections." But he ignores the reasons many reject that explanation of Mrs. Piper's successes. Are there grounds for thinking that Mrs. Piper's access to gossip *actually* played a role? Augustine does not mention any. Moreover, there are many instances throughout Mrs. Piper's career where allegations of fraud or cryptomnesia are particularly implausible. But Augustine ignores those as well. In fact, he ignores William James's comment that Mrs. Piper "showed a most startling intimacy" with sitters' family affairs, "talking of many matters known to no one outside, and which *gossip* could not possibly have conveyed to her ears" (James, 1886, pp. 15–16). Similarly, he ignores the impressive successes of Mrs. Piper's G. P. communicator. Thirty of the 150 sitters introduced to G. P. were people known to the living George Pellew, and G. P. recognized twenty-nine of them. The thirtieth, whom he failed to identify at first, was someone who had grown from a girl to a woman since the last time she saw the living G. P. The G. P. communicator interacted appropriately with these sitters, and he seemed to know a great deal about their lives and relationships with Pellew. It is both illuminating and refreshing to compare Augustine's cursory dismissal of Mrs. Piper's mediumship to accounts by Alan Gauld, who dives deeply into the small and often revealing details and painstakingly evaluates them (Gauld, 1982, 2022).

Perhaps Augustine believes he is under no obligation to consider apparently positive evidence of Mrs. Piper's paranormal abilities—presumably on the grounds that conventional explanations have already been vetted by the scientific community and must therefore always be chosen over unconventional ones. But whether or not that is the case, Augustine's flippant appeal to gossip is a poor excuse for an explanation of Mrs. Piper's mediumistic success. At best, it is a promissory note for an explanation. If Augustine wants to dismiss Mrs. Piper's abilities, he must demonstrate that his gossip hypothesis has some evidence in its favor, and also that it is adequate to a wide range of facts. But Augustine avoids mentioning—much less discussing—evidence favorable to Mrs. Piper.

Granted, Augustine mentions that private detectives tailing Mrs. Piper never found anything suspicious. But he is mute on the significance of the many times Mrs. Piper

got intimate hits with anonymous sitters she was meeting for the first time—including proxy sitters and people who, during the medium’s visit to England, happened to be travelling through Cambridge. So although it is certainly relevant that Mrs. Piper was never caught cheating, survivalists do not need to rely on a never-caught-cheating card. Augustine simply ignores the strongest reasons for thinking that cheating is highly improbable.

With regard to point (2) above, there are serious reasons for relaxing our commitments to standard interpretations of the neurophysiological data and entertaining possibly radical alternatives. Some famous experiments in the 1920s by psychologist Karl Lashley illustrate this clearly. Lashley thought he knew where memories would be stored in a rat’s brain. But he found that no matter how much of a rat’s brain he surgically removed, trained rats continued to run their maze. And when Lashley reached the point in his surgical marathon where the poor critters were unable to run a maze, they were unable to do *anything* (Lashley, 1929). So some—but not Lashley—concluded that a rat’s memory is not localized at a specific place in the rat’s brain. Rather, memories are *diffusely* localized, much as information is diffusely distributed in holograms.

This proposal catapulted Karl Pribram to the status of a pundit.² However, to someone not antecedently committed to the received wisdom about mind–brain relations, Lashley’s experiments take on a different sort of significance. They suggest that memories are not located *anywhere* or in any form in the brain. More generally, they suggest that the container metaphor (that memories and mental states generally are *in* the brain or in something else) was wrong from the start, because memories (and mental states generally) are not *things* or *objects* with distinct spatiotemporal coordinates.

But this takes us into deeper metaphysical waters than we need right now. What is important here about the Lashley example is that it illustrates, first, how possibly unrecognized assumptions undergird our understanding of Nature, and also how those assumptions infiltrate our ways of speaking. That is why scientific reform can initiate linguistic reform. We will return to this topic below, when we consider McTaggart’s position.

As Augustine noted, the BICS Rules and Regulations informed entrants that “BICS will accept evidence and eyewitness testimony supporting the legal requirement that establishes proof beyond a reasonable doubt.” Although there is much one could say about the appropriateness of that legal requirement in survival research, Augustine goes in a different direction. He cites the work of Elizabeth Loftus (Loftus, 1979), and he claims that “seminal research into the reliability of eyewitness testimony provides all sorts of reasons to hesitate to rely upon it so heavily (as survival

research typically does).”³ However, Augustine seems unaware that *in that same work* Loftus actually made a solid argument *against* the view that eyewitness testimony is generally unreliable and malleable. We will look at the argument shortly.

It is easy to see why one might worry about the reliability of first-person observation or memory reports—perhaps, reasoning as follows: Some first-person reports are clearly unreliable and malleable. For example, visual illusions, pareidolia, etc., are both common and quite real, and they make it easy to misinterpret what one is experiencing. Similarly, in *staged incident* experiments, subjects are taken by surprise to witness a carefully prearranged event such as a confrontation or dispute. But when asked what they saw, they usually get some critical details wrong—for example, who pulled out a gun first. Moreover, memory reports can easily fall victim to the gnawing tooth of time. These errors can only further reduce the veracity of testimony generally.

But that argument misses the point. Visual illusions, staged incident errors, and so on do not diminish the veracity of reports from the *strongest* cases where (for example) the light was good, the investigators knew what they were doing and were experienced in detecting fraud, specific measures were taken to minimize the possibility of fraud from the start, the phenomena occurred slowly enough to permit careful close-up examination, and witnesses had plenty of time to examine the setup and could monitor the phenomena closely while they occurred. That is why Crookes’ accordion test with D. D. Home and the 1908 Naples sittings with Eusapia Palladino are so important (see Braude, 1997). Moreover (as we note below), there are good reasons for thinking that memory reports may not be as fragile as many suppose.

At any rate, Loftus argued sensibly for the view that people remember certain types or details of events better than others. She noted that experiments have confirmed the commonsense observation that eyewitness reports are more reliable when the perceived events or objects are observed repeatedly or for extended periods (Loftus, 1979, pp. 24–5). Thus, she approvingly quoted D. S. Gardner’s observation that

The extraordinary, colorful, novel, unusual, and interesting scenes attract our attention and hold our interest, both attention and interest being important aids to memory. The opposite of this principle is inversely true—routine, commonplace and insignificant circumstances are rarely remembered as specific incidents. (Loftus, 1979, p. 27)

That is why one can argue plausibly that in the *best in-*

vestigations of physical mediumship—the ones that matter—conditions of observation were actually *conducive* to reliable eyewitness reports (Braude, 1997). So we challenge Augustine to (1) demonstrate his command of the details of Crooke’s accordion test with D. D. Home, or the 1908 Naples sittings with Palladino, (2) demonstrate his grasp of the reasons why many believe those details rule out fraud, (3) explain credibly how observers could have been (or were actually) mistaken about what occurred, and (4) explain why we should believe that fraud actually occurred.

A more sophisticated and nuanced view of first-person reports would acknowledge, first, that *all* first-person observation and memory reports are only conditionally, rather than intrinsically or categorically, acceptable. Our decision whether or not to accept a particular report depends on various factors. Some of the most important of those factors are: (a) the capabilities and interests of the observer; (b) the nature of the object allegedly observed; and (c) the means of observation and the conditions under which the observation occurred. In judging the reliability of observation claims or memory reports, we weight these factors differently in different cases. But in general, it matters: (a) whether the observers are trained, sober, honest, alert, calm, attentive, subject to flights of imagination, fortunate enough to have good eyesight, and whether they have any strong prior interests in observing carefully and accurately; (b) whether the objects are too small to see easily, whether they are easily mistaken for other things, or whether they are of a kind whose existence cannot be assumed as a matter of course (e.g., unicorns, pixies, Elvis sightings); and (c) whether the objects were observed close at hand, with or without the aid of instruments, whether they were stationary or moving rapidly, etc., whether the observation occurred under decent light, through a dirty window, in the midst of various distractions, and so on. The best cases of ostensible physical mediumship easily survive such scrutiny (Braude, 1997).

Furthermore, it is worth noting that even though first-person reports are only conditionally acceptable, we rely on them all the time, usually successfully, in our daily commerce with others. Indeed, we *must* do so. As philosopher C. A. J. Coady argued, “our normal cognitive practices are underpinned by our reliance upon what others tell us” (Coady, 1992, p. viii). However, pursuing the topic of testimony further will take us too far afield.

Nevertheless, one final point is worth mentioning. Ironically, a well-known Loftus example of a malleable and false traumatic memory helps make the case for her opponents. She cites baseball pitcher Jack Hamilton’s memory of hitting batter Tony Conigliaro in the face with a fastball. Although Hamilton claimed to remember the event per-

fectly, he remembered it as happening during a day game, when in fact the game was played at night. Of course, it is contentious (to say the least) that the time of day was a “critical” detail (Loftus, 1993, p. 531). But the important point is that Hamilton remembered that he hit Conigliaro in the face. Indeed, one would think that this is precisely the sort of case that would be embraced by the authors Loftus was opposing. After all, the allegedly critical detail concerning the time of the game was not traumatic. Thus, Hamilton’s failure to remember it is compatible with the claim that traumatic or highly unusual or dramatic events leave an indelible impression on the mind. However, the traumatic part of the event, hitting the batter, seems to have been etched in Hamilton’s memory (see Olio, 1994, for a similar observation). So, ironically, Loftus’s strategy adds support for the sensible view that traumatic or dramatic events (like hitting a batter in the face) are more indelible than non-traumatic or less-arresting features of the incidents (such as the time of day). Loftus has certainly not shown that first-person reports are unreliable generally or, in the best cases, easy to dismiss. (For further discussion of Loftus’s dishonesty and confusions, see Braude, 1995, 1998.)

Let us turn now to Augustine’s lopsided focus on alleged experimental failures. Once again, his comments disappoint. For example, he ignores one of the clearest lessons we have learned from parapsychology experiments—namely, that subjects in ESP tests often either focus on something more personally meaningful or interesting than the official target, or at least get distracted by, and focus on, some minor feature of the target, thereby making it difficult to distinguish near hits from misses. The Maimonides dream telepathy experiments provide some dandy examples (see Ullman, et al., 2002). But of course, this may also occur with any ESP test, including Augustine’s favored combination-lock tests of survival, where it is especially easy to imagine more arresting targets.

Imants Barušs provides another example:

It was found in the PEAR research that remote viewers were often distracted by more interesting objects than the official target . . . [For example] in 1993, during a class field trip to the PEAR lab, one of the students went to New York City for the evening . . . She chose the Empire State Building while we tried to remote view where she was. I saw her at a small Catholic church in a square with pigeons walking outside. She and her friend sat in the pews on the right hand side. There was a slimy guy in the back of the church who freaked them out, so they left. That was all correct, except that it occurred later in the evening, not at the time we

were remote viewing back in the lab at Princeton. This is an example of a more interesting target drawing a remote viewer's attention than an official target. (personal correspondence, 4/19/2022)

At one point Augustine counters the survivalist's reliance on first-person accounts by saying "survival agnostics might well note that there is an abundance of eyewitness reports for the existence of the Loch Ness Monster, too, that they find just as unconvincing." He is specifically concerned here with Michael Nahm's questionable contention that the reliability of testimony is enhanced when there is agreement among a multiplicity of observers. But Augustine did not seize the opportunity to note that conditions of observation matter more than the number of witnesses. In the most reliable first-person reports in parapsychology, conditions of observation are actually conducive to accurate reporting. For example (as we noted earlier), in the best cases of physical mediumship, observers can study the phenomena closely while they are occurring, and experimental controls can make cheating improbable. Obviously, there is nothing comparable in alleged Nessie sightings—certainly no close-up observation.

Furthermore, the strongest cases of physical mediumship, and macro-PK in general, do not seem vulnerable to concerns about the fallibility of memory over time. For one thing, observers sometimes write their accounts at the time or shortly thereafter. And the shocking or dramatic phenomena described in those reports seem to be of the kind that Loftus and Gardner regard as conducive to reliable first-person reports.

It is curious how Augustine fails to acknowledge that our description of Nature rests on independently unverified presuppositions, required simply to get any inquiry off the ground. As the history and philosophy of science demonstrate, those assumptions are not sacrosanct, and they can later be abandoned and replaced. But Augustine seems to think that anti-survivalist or survival-agnostic judgments about the significance of empirical data are assumption-free, or at least freer than the claims made by survivalists. That is why he repeatedly appeals to "what the [neuroscientific] data tell us," as if one could grasp what the data mean independently of any deeper assumptions, or perhaps as if Augustine's assumptions—and the entire conceptual and scientific framework of which they are a part—are somehow unassailable. For example, he writes,

Negative outcomes are only frustrating if you want the experiments to come out a certain way. In lieu of remaining frustrated by failing to get the data that you were hoping for . . . survival researchers would better serve *science* by setting

aside their feelings and heeding what the *data* are telling them.

Let us set aside the snarky and condescending tone of this passage. It is enough to note that it is one of several instances in which Augustine either seems blind to his own first principles or else treats them as somehow privileged.

It is particularly disappointing that some of Augustine's arguments are variants of familiar superficial attacks on parapsychology. Augustine repeatedly claims that survival researchers have a long track record of failures when trying to elicit evidence of survival, as if that lends support to the view that there is no evidence of survival *at all*. But he does not consider whether test conditions might inhibit performance, and he greatly underestimates how tricky it is generally to determine ahead of time how a parapsychological test or experiment will turn out. That trickiness is not surprising, considering that researchers do not know what kind of human ability they are trying to wrestle from a real-life setting and then study under artificial controls or other novel background conditions. But then they do not know whether their planned experiments are even appropriate to the phenomena, and also whether the demands of the experimental setting tend to frustrate the quest for positive results. Of course, Augustine also lacks that knowledge, but—apparently undaunted—he nevertheless purports to know what we should expect to find if the phenomena under investigation are real.

We have seen that anti-survivalists must do more than assert that evidence suggesting survival can be accounted for by appealing to the possibility of fraud or other Usual Suspects. They must wallow in the grubby details and show that fraud (or whatever) is either likely or actual. But we need to be fair here, because survivalists have an analogous duty. In order to explain away or dismiss experimental failures, they must do more than appeal to the mere possibility of psi-inhibitory conditions. They must also provide reasons for thinking that those conditions were actually or probably obtained. And if they fail to mount that defense, then critics can justifiably complain that survivalists do not take experimental failures as seriously as they would take successes.

In any case, not all parapsychological tests (including survival investigations) have failed. For example, although Mrs. Piper's results are often ambiguous and messy, the investigation of her mediumship counts as a failure only on an indefensibly strict standard of success, one which we reject in many domains. That is why a baseball player who gets a hit 1 out of 3 times is considered excellent.

Furthermore, we *know* that most (if not all) human capacities are situation-sensitive. And we know that even the best modern or contemporary remote viewers (e.g., Joe

McMoneagle, Ingo Swann, and Pat Price) do not always get a hit. But then contrary to what Augustine suggests, a test subject's misses—even consistent misses—do not clearly (if at all) cast doubt on the reality of the phenomenon under investigation. We also have good reason to believe that if psychic abilities are real, their manifestations may be disguised and subtle—for example, in the interest of our psychological well-being (see, e.g., Eisenbud, 1970, 1982, 1992). But Augustine does not even entertain that option, much less evaluate it carefully. Instead, he again avoids discussing evidence suggesting survival at the level of detail and sensitivity to the nuances of human behavior that the best cases deserve.

Benjamin Franklin once quipped: “Clean your finger before you point at my spots.” We have noted that one of Augustine's main tactics is to avoid discussing psychic successes and to concentrate instead on failures to elicit psi on demand—for example, unsuccessful efforts to get OBEs and NDEs to identify remote targets. Yet he accuses survivalists of cherry-picking evidence favorable to their position. Furthermore, Augustine seems to infer not simply that nothing psychic was happening during the tests of OBEs and NDEs, but more likely, given his broad skepticism about things paranormal, that nothing psychic *could* occur. Regrettably, Augustine never clarifies this. But he has a recommendation. He urges us to attend to *what the data tell us*, and when we do that,

One possibility stands out among the rest for its sheer simplicity: perhaps out-of-body experience (OBE) adepts and near-death experiencers (NDEs) cannot describe remote visual targets under controlled conditions because nothing leaves the body during OBEs or NDEs that could perceive them.

But that is hardly the only—or most plausible—interpretation of the negative results. And even if we agree that nothing leaves the body in these cases, that is not enough to support a more sweeping skepticism about the reality of psychic functioning in general, or about the specific ability to accurately describe remote targets. One of the few things we know about psychic abilities—in addition, we would say, to their existence—is that they are psychodynamically complex. Moreover, we know that RV superstars often display distinctive RV abilities and lack others. For example, Ingo Swann was able to direct the operator of a mini-submarine to the location of a sought-for and previously undiscovered ship wreck. And Hella Hammid (also in the sub) successfully described the objects they would find at the site. But apparently Ingo and Hella were unable to exchange the tasks. We also know that the ability to

demonstrate ESP or PK *reliably* seems to be quite rare, even if psychic experiences can occur to virtually anyone under the right conditions. But then we must exercise caution in interpreting a parapsychology experiment's negative results. Augustine presumably knows this, but he nevertheless fails to consider what kind of ability is under investigation. He does not even entertain the counter-proposal that when OBEs and NDEs fail to identify remote targets in formal tests, perhaps they are simply not particularly good at it—or good at it in formal tests or under mental or physical duress.

After all, there is no evidence that people generally, or randomly selected people, are good at remote viewing, or as good as the small number of outstanding RVers. But then we can say, plausibly, that the ability to remote view is genuine (as RV superstars demonstrate), but like many normal abilities it is not widely or evenly distributed, and it is also situationally fragile. That is what the data, both negative *and positive*, tell us.

Augustine apparently considers encrypted messages and combination-lock tests to be the gold standard for testing mediums. But he claims repeatedly that all such tests have failed. He writes, “While some mediums were asked to describe the contents of sealed envelopes or provide auditory information, most direct tests of survival involve asking living persons to posthumously reveal to a medium key words, phrases, or mnemonic devices, ostensibly unknown to any living person, that would decipher encrypted messages or open user-set combination locks.” Then a few sentences later, “After 121 years of such simple tests, only undeniably fraudulent mediums (Spraggett & Rauscher, 1973) or cryptologists (Bean, 2020; Gillogly & Harnisch, 1996) have ever been able to solve them.” Predictably, Augustine does not consider the option that the tests were psi-inhibitory. He also does not indicate what his position would be if the tests were successful. Would he concede that the positive results count as evidence of survival? That would help clarify how open-minded he is about evidence of the paranormal.

Augustine also approvingly cites magician Christopher Milbourne's claim that “Many brilliant men have investigated the paranormal but they have yet to find a single person who can, without trickery, send or receive even a three-letter word under test conditions” (1970, p. 37). Now that may have been true in 1970, but since then James Carpenter conducted an experiment that successfully transmitted the word “Peace” (Carpenter, 1991). However, even if Milbourne's claim had been true today, what would it have shown? As we have noted, Augustine does not consider the strongest features of the cases he discusses. He simply focuses on ways in which a case like that of Mrs. Piper falls short of an ideal. But then, when we take into

account not just the strongest evidence of remote viewing abilities, but the totality of evidence in parapsychology, the failure of OBEs and NDEs to succeed in formal or controlled tests is ambiguous and certainly not impressive or clear enough for us to conclude that the subjects totally lack the ability being tested. And it fails even more clearly to support a general skepticism about the reality of paranormal phenomena.

Lurking below the surface is an interesting and serious problem which Augustine does not consider at all—namely, whether we can ever confidently assess success or failure in *any* parapsychological test. For one thing, those who originally designed encrypted message or combination-lock tests were making various assumptions about what it is like to survive death—for example, whether (or to what degree) the channels of communication are noisy, and whether the deceased would even care about communicating with the living.⁴ But then, test failures would at best only disconfirm a particular *model* of personal survival. Moreover, this problem is an instance of a more general and very serious difficulty faced by both survivalists and anti-survivalists—namely, that most (or perhaps all) of the time, we have no idea what is really going on in a parapsychological experiment.

Parapsychologists try to study phenomena which, if real, could apparently subvert any experimental controls. Yet researchers too often assume, tacitly and naively, that subjects will use only the psychic ability being investigated, that they will use that ability only after the experiment has begun, and that experimenters, judges, and mere bystanders will use no psychic abilities at all to influence the outcomes. But this supposed “gentleman’s agreement” as Eisenbud (1963, 1992) called it, is clearly preposterous. We have no reason to think that people will be so well-behaved in exercising their psychic abilities, and we also have no idea what issues or hidden agendas might be on the minds of participants and onlookers, perhaps motivating them to influence the results of the tests (Braude, 1997). This is the recalcitrant problem of sneaky or naughty psi. Unfortunately, we cannot pursue it here.

We know that evidence of paranormality can be captured even in a skeptical environment, especially if investigators act sympathetically and respectfully, as in the painstaking 1908 Naples sittings with Palladino (Feilding et al., 1909). We should also recall that the parapsychological community has a very good track record of identifying colleagues who are psi repressive (e.g., John Beloff) and psi conducive (e.g., Helmut Schmidt). And there is good reason to believe that this difference has much to do with the experimenters’ beliefs, personality, and the quality of their interactions with subjects.

However, Augustine’s assessment of encrypted

message and combination-lock tests seems viable only when we regard both experimenters and subjects as psychological stick figures, unburdened by self-defeating character traits and untroubled by the concerns, fears, hopes, and other frailties that plague most of humanity. The issues here coincide with some of those discussed in connection with the replicability problem in parapsychology (see Braude, 2018).

In any case, Augustine’s narrow focus on subjects’ failures to get hits is very much in the spirit of a foolish claim made by psychologist C. E. M. Hansel in the 1983–84 BBC documentary “The Case of ESP.” Hansel said that to demonstrate telepathy, one need only tell him what he is thinking. But we wonder what Hansel would have said had he been challenged to personally demonstrate penile erection, then and there, on camera and on demand. If Hansel failed that test, would he have been logically compelled to admit that he just cannot “get it up”?

It is no secret that abilities vary distinctively from one person to another. For example, some porn stars could probably ace the challenge we imagined presenting to Hansel. Also, one cannot infer that people have a certain ability (e.g., to compose music, or psychokinetically raise a table) just because they have certain other abilities in the same general domain (e.g., to play the trumpet, or psychokinetically nudge a matchstick). Savants illustrate this dramatically. Calendar calculators tend to be accurate only within specific ranges of years, and those ranges differ from one individual to another. And although calculators might be able to perform rapid and complex operations concerning dates or remember extremely long numbers, they might be unable to do simple addition or change a dollar bill. The famous calculating twins, George and Charles, amused themselves by exchanging 20-digit prime numbers, and they could factor any number presented to them, but they could not count to 30 (Sacks, 1985). Another savant could rapidly solve complex algebraic problems in his head, but he seemed unable to comprehend even simple principles of geometry (Treffert, 1989).

Along the same lines, our intuitions about what to expect from a person—either generally or in specific circumstances—are notoriously unreliable. Consider: We know that good hypnotic subjects can make themselves anesthetic in response to suggestion. And we also know that this ability can *take forms no one predicted*—e.g., becoming anesthetic in an area corresponding to no natural anatomical region (e.g., in the shape of a band around the arm) (see Janet, 1901; Myers, 1903). Similarly, David A. Oakley observed that “conversion symptoms defy the normal rules of neuroanatomy and neurophysiology” and that “hypnotically suggested anaesthesia of a hand . . . will typically show a glove pattern with sharply defined boundaries in

apparent correspondence to a naïve understanding of sensory innervation patterns” (Oakley, 1999, p. 244).

Augustine seems to adopt yet another familiar, and tawdry, skeptical strategy. It is revealed in the following passage: “DRW also inform us that ‘Fraud was never detected in’ the early 20th-century Kluski molds (wax casts of human hands), even though plausible normal ways of producing them are not hard to come by.” But that is as far as Augustine takes his discussion of Kluski. He fails to provide even a single example of a plausible normal counter-explanation of the Kluski molds, and as usual, he does not discuss the strongest reasons for rejecting skeptical counter-proposals. Skepticism is easy when one ignores relevant details.⁵ For a better-informed and more balanced discussion of Kluski, see Weaver (2015).

Similarly, Augustine writes, “The history of exposures of [mediumistic] fraud . . . the typical need for darkness in order for the phenomena to manifest . . . and the likely use of skills to help produce effects, all of which DRW note, ‘ought reasonably to beget a suspicion against all relations of this kind.’” But since he consistently ignores the cases that most effectively resist the easy appeal to fraud and poor controls, Augustine comes uncomfortably close to another familiar and worthless skeptical claim—namely, that the results of parapsychology experiments should be rejected as long as fraud is possible. But of course, fraud is possible in any experiment and in any branch of science. What matters is not whether fraud is possible, but whether it is actual, and whether (or to what extent) the evidence for a properly conducted experiment or investigation outweighs the evidence for fraud.

Moreover, although there is a clear and rich history of mediumistic fraud, and although that history illustrates why experimenters must exercise caution and impose good controls, one cannot generalize from tainted cases to impugn the entire body of mediumistic evidence. That is one reason why the strongest cases are the ones that matter. And of course, to illustrate why those cases matter, one must look at their details and consider (say) what we know about the experimenters and experimental controls. But that requires considering apparently positive evidence at a level of detail that Augustine consistently avoids.

Another passage reveals Augustine’s all too easy reliance on undefended assumptions. “The absence of clear-cut permanent paranormal objects produced by physical mediums should clue in any reasonable person of the dubious reliability of this phenomena [sic] as a source of evidence for the paranormal in general, let alone for discarnate personal survival.” Apparently, Augustine assumes that a physical medium’s paranormally produced objects should at least sometimes be permanent, or perhaps that they should be more common. But why? We find no argu-

ment for those assumptions, and they are certainly not obviously true. Augustine comes uncomfortably close to presumptuously dictating to Nature the forms in which he will accept her secrets.

In any case, if apported phenomena are genuine, they would be examples of permanent paranormal objects. That would be true even if (as some maintain) the apported objects existed previously at another location. Perhaps Augustine would simply dismiss the evidence of apportation. But that would be irresponsible. The serious literature on apportation, while not extensive (see, e.g., Nahm, 2019; von Ludwiger & Nahm, 2016), deserves a careful, open-minded evaluation, and it should range over both mediumistic and poltergeist contexts.

Another revealing passage is the following:

Nahm disregards the *absence* of reports of intermission memories in the vast majority (~80%) of CORT. That is, in taking the existence of any intermission “memories” to be evidential, he disregards the more specific issue of why there are so few of them. On the face of it, *if one can really remember aspects of an even older past life, then one should (usually) also be able to remember aspects of a more recent (and perhaps half-a-century-long) intermission period between that life and the current one, all else held equal (assuming that before-life memories function like those already known to exist, anyway).* (italics added)

This is a good example of how unverified, controversial assumptions can be enlisted when convenient. What is the basis for Augustine’s claim italicized above? Why assume this is how before-life memories operate? We barely understand how memory works in *this* life, and we are still puzzled by the memory anomalies displayed by mnemonists and savants, as well as people suffering from dementia. Augustine once again understands and criticizes the way survivalists often import unstated and undefended assumptions into the debate, but he apparently misses his own frequent deployment of the same strategy.

This reminds us of another, related, example, showing how controversial assumptions are unavoidable when trying to interpret the evidence for survival. In a *JSE* paper published in 2000, David Bishai addressed the familiar anti-survivalist argument that “reincarnation appears to be refuted by population statistics” (Edwards, 1996, p. 227). He sketched a simple “circular migration model” that accounts for the data from a reincarnationist perspective, and he showed that different assumptions about the “dwell time” between incarnations yield different predictions about the peak of human population growth. But more important,

Bishai showed that metaphysical assumptions are unavoidable no matter where one stands on the issue of reincarnation and population growth. Specifically, he noted that the alleged incompatibility between the reincarnation hypothesis and the facts of population growth rests on the very controversial assumption that “the mean duration of stay in the afterlife has been constant throughout human history” (Bishai, 2000, p. 419). Presumably, Edwards was unaware that his own position rested on that assumption. Ironically, then, Edwards’ purportedly hard-nosed and condescending attack on reincarnation is as deeply and inevitably metaphysical and debatable as the view he opposes.

McTaggart and the Significance of Physiological Data

Survivalists maintain that we, or something essential to who we are (our mind or soul), can persist even when our bodies die. And Augustine believes this puts survivalists in an awkward position empirically, because they cannot explain away a large and respectable body of neuroscientific data suggesting that survival is impossible. Now we agree with Augustine that the study of survival must respect the data, whatever the data might be. And we also agree that some evidence seems *prima facie* to cut against the survival hypothesis. Indeed, we agree there is a huge body of research pointing, at least on the surface, to the apparently intimate connection between brain states and mental states—and in particular, the causal dependence of the latter on the former. So Augustine is justified in insisting that survivalists confront the challenge posed by the evidence of mind–brain correlations. However, he again resorts to straw-man tactics, making the following condescending criticism.

According to *what principled reason*, then, can we rule the neuroscientific evidence as inadmissible? Not wanting to deal with powerful counterevidence is not an epistemic principle, but a fallacy (**confirmation bias**). Failing to deal with it shirks one’s epistemic responsibilities; it is merely aiming to confirm what one wants to hear, not seeking the truth.

Similarly, he writes, “concomitant [mind–brain] variations *are* evidence, no matter how staunchly empirical survivalists fight to the death to pretend otherwise.”

But who, exactly, is engaged in this epic struggle? Who believes that concomitant mind–brain variations are not evidence? Augustine again offers an implausible or inaccurate characterization of his survivalist opponents. Moreover, he seems simply to have missed the point. Sur-

vivalists need not consider the correlations to be non-evidential. But what are they evidence of? Augustine’s anti-survivalist position is only an option, and probably it seems compelling primarily to *those antecedently committed to, or caught in the grip of*, a prevailing conventional scientific view of the world. Similarly, who is actually guilty of claiming that neuroscientific evidence is *inadmissible*? In fact, its admissibility is precisely why survivalists make the effort to find viable alternative accounts of the data! Moreover, Augustine ignores the strongest reasons for thinking that the best cases *cannot* be explained away in conventional scientific terms. Indeed, conventional science has *already* failed to accommodate the most robust evidence in parapsychology. So it is not as though there are no chinks in that armor.

At any rate, survivalists must say something about how mental states or characteristic chunks of personal psychology might persist in the absence of brain activity. More specifically, they must explain why, if mental states can occur independently of bodily states, they *seem* in so many respects to be bodily dependent. Typically, survivalists do this by arguing that the brain is merely one kind of physical *instrument* for expressing mental activity.

Predictably, most anti-survivalists find that hard to swallow (e.g., Edwards, 1996), and Charles Richet offered an analogy to explain why (Richet, 1924). In doing so, he anticipated a position many neuroscientists and others would probably now express somewhat differently, but no more cogently. Richet observed how certain changes to the brain affect and sometimes seem to obliterate memory. And he noted that survivalists regard the brain as “only an instrument, which is unable to respond unless it is intact” (Richet, 1924, p. 109). Although Richet did not object to that claim, he found it incredible to assert further that this instrument is not necessary for memory and other cognitive functions. He wrote,

It is as if I were to say that in an electric lamp the passage of the current and the integrity of the mechanism of the lamp are not necessary for the production of its light. (p. 109)

Many find that analogy, and others like it, to be very seductive, and Augustine apparently thinks they pose an insurmountable challenge to the survivalist. However, the analogies are likely to be irresistibly seductive only (or primarily) to those who have already internalized the conventional wisdom regarding mind–brain relations. But, as we will see below, those who regard the brain as an instrument mediating the expression of mentality can approach the matter from a much different perspective.

Regrettably, Augustine ignores some intriguing argu-

ments from philosopher J. M. E. McTaggart, showing how survivalists can transform the prevailing scientific perspective on the data into something more survival-friendly. Notice first that survivalists hold that the self—whatever, exactly, it may be—is not something identical with one’s physical body or a part of the body (e.g., the brain). Nor is it something totally causally dependent (or supervening) on part of one’s physical body. Instead, survivalists could say that the self, whatever exactly it is, and as we know it both introspectively and through our earthly interactions with others, is something that *has* a body.

Now we realize that this locution may strike some as intolerably quaint at best, and possibly question-begging at worst, since it may presuppose precisely what is at issue: namely, that the self might not be embodied. However, survivalists must be allowed to use the locution that the self has a body. *Pre-theoretically*, it is no less legitimate than the competing, and equally theory-laden, terminology of anti-survivalists. Moreover, if survivalists are right, then reality is profoundly different from what conventional science proposes, and one would expect that to require some modifications to our usual ways of speaking.

Granted (and as Augustine notes), a great deal of evidence of mind–brain correlations seems to cast doubt on the survivalist position. It is precisely what draws many people to some form of the identity theory or epiphenomenalism. But McTaggart argued that survivalists can concede that Richet’s analogy of the electric lamp is forceful and that correlations pose at least an initial challenge to their position. However, survivalists contend that other bodies of evidence exert a theoretical pull in the opposite direction. How, then, can survivalists argue for the superiority—or just the adequacy—of their point of view? According to McTaggart, one strategy would be to offer competing analogies that are at least as weighty as analogies apparently favoring the anti-survivalist. We will consider one such analogy shortly.

Moreover and perhaps most important, McTaggart argued that anti-survivalists make several *unwarranted inferential leaps* when they interpret the evidence. For example, no matter how intimate the mind–body connection seems to be, the data would show, *at most*, “that *some* body was necessary to my self, and not that its present body was necessary” (McTaggart, 1930, p. 104). But even that may be going too far; strictly speaking, the data show us only what *is* the case, not what *must* be the case. Thus, the data do not establish limits on the possible manifestations of selfhood. Specifically, nothing in the data compels us to conclude that a self must be linked to a human body or any kind of physical body. So on an even more circumspect or conservative appraisal of the data, we might conclude simply that “*while a self has a body*, that body is essentially

connected with the self’s mental life” (p. 105). McTaggart argued,

. . . it does not follow, because a self which has a body cannot get its data except in connexion with that body, that it would be impossible for a self without a body to get data in some other way. It may be just the existence of the body which makes these other ways impossible at present. If a man is shut up in a house, the transparency of the windows is an essential condition of his seeing the sky. But it would not be prudent to infer that, if he walked out of the house, he could not see the sky because there was no longer any glass through which he might see it. (p. 105)

McTaggart makes a similar point with regard to the more specific, and apparently intimate, causal relation between brain states and mental states.

Even if the brain is essential to thought while we have bodies, it would not follow that when we ceased to have brains we could not think without them . . . It might be that the present inability of the self to think except in connexion with the body was a limitation which was imposed by the presence of the body, and which vanished with it. (p. 106)

McTaggart’s view is insightful. Strictly speaking, the evidence for mind–brain correlations does not show that selfhood or consciousness is *exclusively* linked to bodily processes, much less the processes of any particular physical body. We noted earlier that survival-unfriendly interpretations of the neurophysiological data may seem initially compelling because their presuppositions are widespread and deeply rooted. And if so, it may be a useful intellectual exercise to try to divest ourselves of those presuppositions and then take a fresh look at the data. We might find, then, that McTaggart’s (or some other survivalist) interpretation seems more immediately appealing. It is therefore regrettable that Augustine does not rise to the challenge.

Moreover, it is not clear to what extent anti-survivalists can legitimately cite neurophysiological data in support of their position. After all, in the debate between survivalists and anti-survivalists, *both* positions are in question. In fact, one would *expect* survivalists to consider alternative interpretations of the neurophysiological data apparently unfavorable to their position. After all, data do not come pre-interpreted. They must always be evaluated in the light of a background theory (or a set of basic presuppositions). Often enough (and as we saw earlier in connection with

Lashley), what we take to be obvious interpretations of the data may reveal more about our unexamined theoretical presuppositions, or lack of imagination, than they do about the phenomena in question.

Augustine offers a list of “agreed-upon facts that scientists have discovered about the mind’s link to the brain,” and he maintains “that the chiefly neuroscientific data constitutes *evidence* against discarnate personal survival (and strong evidence at that).” But some of the items on the list are contentious, and all seem to be precisely what we would expect if the brain is merely an instrument for the expression of mentality. The alleged facts are:

1. Minds mature as brains mature
2. Childhood mental development halts when childhood brain development halts
3. Minds degenerate when brains degenerate (due to old age or traumatic brain injury)
4. Creatures with simple brains have simple minds
5. Creatures with complex brains have complex minds
6. Sickening/injuring the brain sickens/injures the mind
7. Mental dispositions can be inherited from one’s parents
8. Mental desires can be induced or eliminated by brain stimulation
9. Mental disorders can be cured by altering brain chemistry with drugs
10. Mental disorders can be brought on by altering brain chemistry with drugs

But this list will not stop survivalists in their tracks. Consider: Advocates of the brain-as-instrument view would presumably be quick to embrace items 1–6 and 8. From their point of view, as the brain develops (or deteriorates), what can be expressed though it changes accordingly. Similarly for 9 and 10: We could equally say that drugs correct, impair, or simply modify the working of the instrument for expressing mentality. And the remaining item on the list:

7. Mental dispositions can be inherited from one’s parents

may not state a fact at all. We wonder: Why not say “learned/absorbed” rather than “inherited”? Although some genetic data suggest that certain personality traits and talents are inherited through DNA, Augustine cannot simply assume that this alleged regularity is an example of nature and not nurture. After all, many (perhaps most) families do not exhibit this generational continuity. In fact, children often have attitudes, dispositions, and preferenc-

es that conflict with those of their parents. Are we really to believe that Augustine does not know this?

Finally, one cautionary terminological point merits a few comments. On the list above (in fact, throughout his paper), Augustine mentions various alleged causal connections between two things: the brain and the mind. Now, we know what the brain is; it is a squishy physical object. But the *mind* is not clearly an object at all. We know that various physical states seem to influence mental states and behavior. But the “mind” (like “personality” or “character”) is merely a shorthand, a general term, for a class of mental events, just as “the weather” is a general term for the class of meteorological events, and “the economy” stands for a class of financial transactions. There is certainly no need, and arguably no justification, for reifying the mind, weather, or the economy. Moreover, thinking of the mind as an entity—a piece of ontological furniture—surreptitiously tilts the discussion in favor of the anti-survivalist. It suggests, right from the start, that the mind and brain are on the same ontological level—or at least that they are neighbors. And no doubt that encourages some to identify the mind with the brain, or at least to posit intimate causal relations between them.

Summing Up

Augustine has squandered an opportunity to advance the debate over survival. What’s needed are novel proposals, not the tired and transparently defective skeptical arguments on which he often relies. As far as dialectical and conceptual sophistication are concerned, Augustine’s critique could have been written in the 1950s and 60s, when arguments similar to his were all the rage among psi skeptics.

As we noted at the beginning of this reply, Augustine makes some reasonable comments on the BICS contest and the winning papers he selected for discussion. But much of the time he offers arguments which can only seem credible to someone ignorant of the relevant evidence. No wonder, then, that Augustine so often fails to mention the strongest reasons for rejecting his charges or suggestions of fraud, malobservation, and other Usual Suspects.

Moreover, we have also seen that Augustine avoids discussion of clearly relevant data or lines of argument that challenge his point of view. Indeed, that may be his principal dialectical strategy. And although he charges survivalists with straw-man reasoning, that is something he often does himself, either by describing the opposing survivalist position in perhaps its least plausible form, or by simply charging survivalists with positions they (or at least the best of the lot) do not hold.

Hopefully, we will eventually see commentaries on the



BICS contest that grapple more constructively with the issues, and which demonstrate a more thorough grasp of the relevant empirical and philosophical landscape.⁶

NOTES

- ¹ Readers may wonder exactly how five authors collaborated on writing this reply. In order to express our comments in one “voice” (so to speak), Braude did the writing. But the text incorporates many corrections and suggestions from the co-authors, which led to numerous revisions, perhaps setting a world record.
- ² Granted, most neuroscientists are unfamiliar with the logical and conceptual errors in positing memory traces. Like Pribram, when confronted with challenges to their views on memory, their first impulse is to simply modify the nature of the trace (say, as a dense neural network) and ignore the reasons for regarding trace theory as deep (or disguised) nonsense. Moreover, the arguments for the vacuousness of trace theory are hardware-independent. No matter how they are configured, it is relatively easy to show that memory traces are impossible objects. For more details, see Braude (2014), “Memory without a Trace.”
- ³ It is unfortunate that both BICS and Augustine use the term “eyewitness testimony.” That term is intimately tied to concerns about guilt or innocence in a legal setting. But first-person reports in parapsychological or survival research do not count as eyewitness testimony in that legal sense. That expression means one thing in a court of law and another when we are considering how to evaluate ordinary first-person observation and memory reports. Hence, our preference for referring simply to first-person reports.
- ⁴ See Sudduth (2014, 2016) for a penetrating discussion of auxiliary assumptions.
- ⁵ Similarly (we hasten to add), belief in survival is easy when one ignores relevant detail.
- ⁶ Many thanks to Michael Sudduth for his helpful comments on an ancestor of this paper.

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